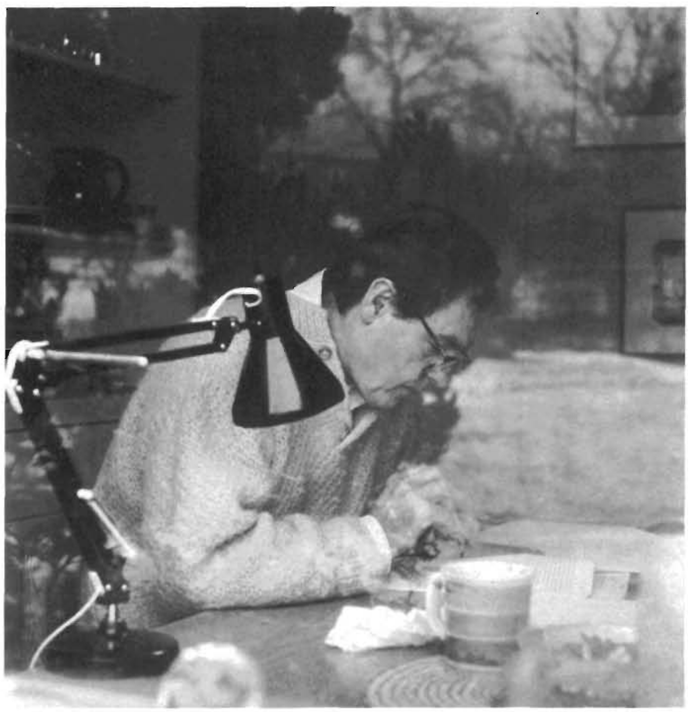
straight man *for* nonlinearity

An interview with Alwyn Scott

When Los Alamos Science *arranged to interview Alwyn Scott, his secretary warned us that if we weren't careful, we'd wind up being the ones interviewed. He listens well. You have a sense he is absorbing everything you're saying and judging by impeccable standards what's valuable and what is not. As Chairman of the Center for Nonlinear Studies, he is popular for this very reason. In fact, all his time could easily be occupied by the myriad projects of the Center were it not for his fierce need to do his own research (he manages this during morning hours at home). Energetic, efficient, enthusiastic, he nevertheless confesses his antipathy for administrative matters. "If you were to look for me at a staff meeting," he comments drily, "I'd be the one doing calculations under the desk."*



Alwyn Scott holds degrees in physics, engineering, and electrical engineering from the Massachusetts Institute of Technology. After earning his doctorate he began teaching at the University of Wisconsin and remained on its staff for twenty years, although his work has taken him at various times to Bern, Switzerland; Naples, Italy; Sendai, Japan; Woods Hole, Massachusetts; Tucson, Arizona; and Copenhagen, Denmark. Throughout his career he has been concerned with the theory of nonlinear wave propagation and its applications; he has written books and holds patents in that field. "My work is sometimes quite theoretical and sometimes experimental . . . my aim, however, is always to bring theory and experiment closer together in applied science." In 1981 Alwyn Scott became Chairman of the new Center for Nonlinear Studies at Los Alamos, where his special interest in solitons in biology has evolved into a project reported in this issue.

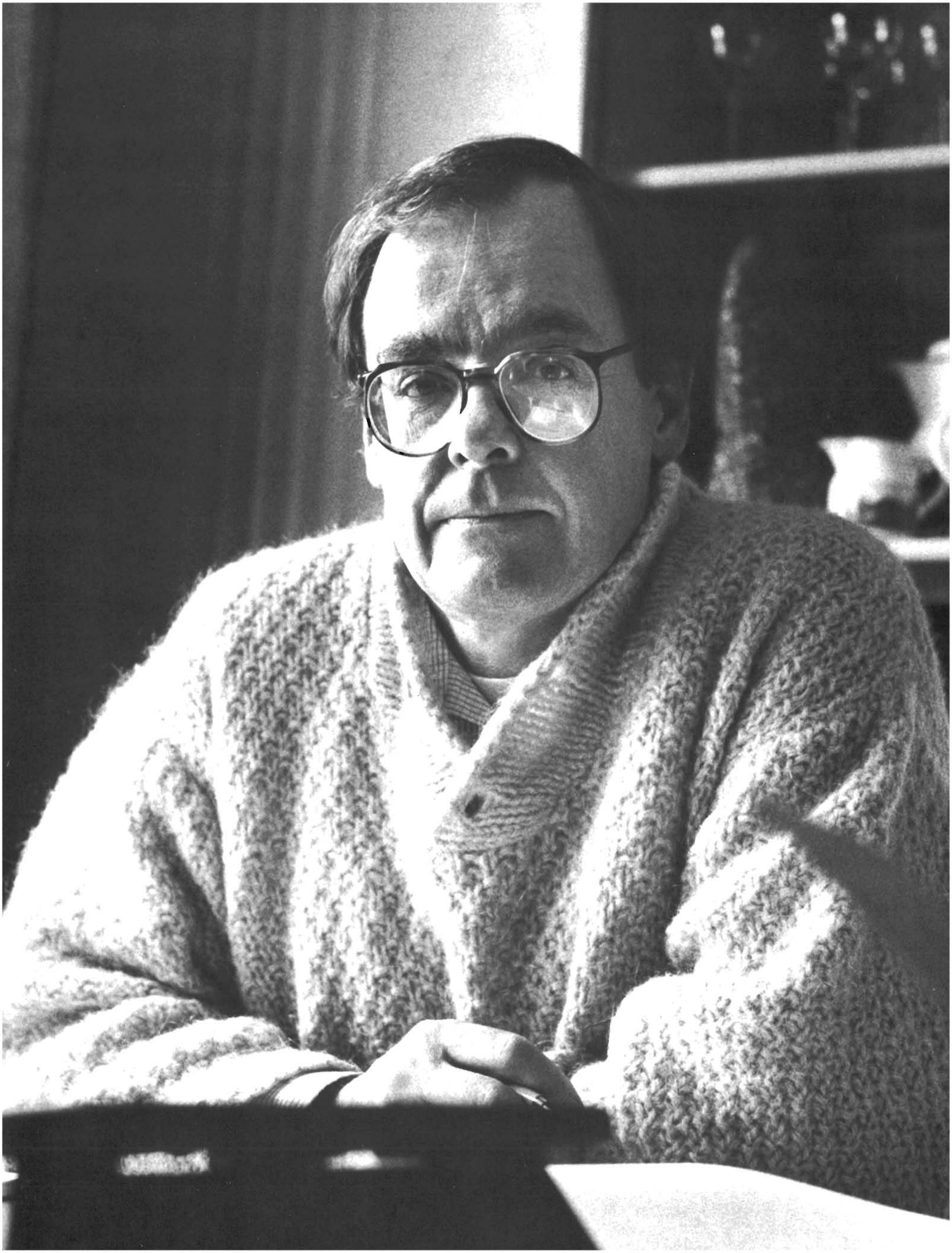
SCIENCE: You've been exploring solitons in biology for quite a while. What's the story behind this project?

SCOTT: In 1978 I attended a soliton conference in Sweden (it was for a week or two out in the country somewhere), and the Soviet physicist Davydov was there. Davydov had originated this work, and he gave a talk on it. He had a model showing how biological energy could be self-trapped as solitons in helical protein and thus provide a mechanism for energy transport and storage.

SCIENCE: Why is that important?

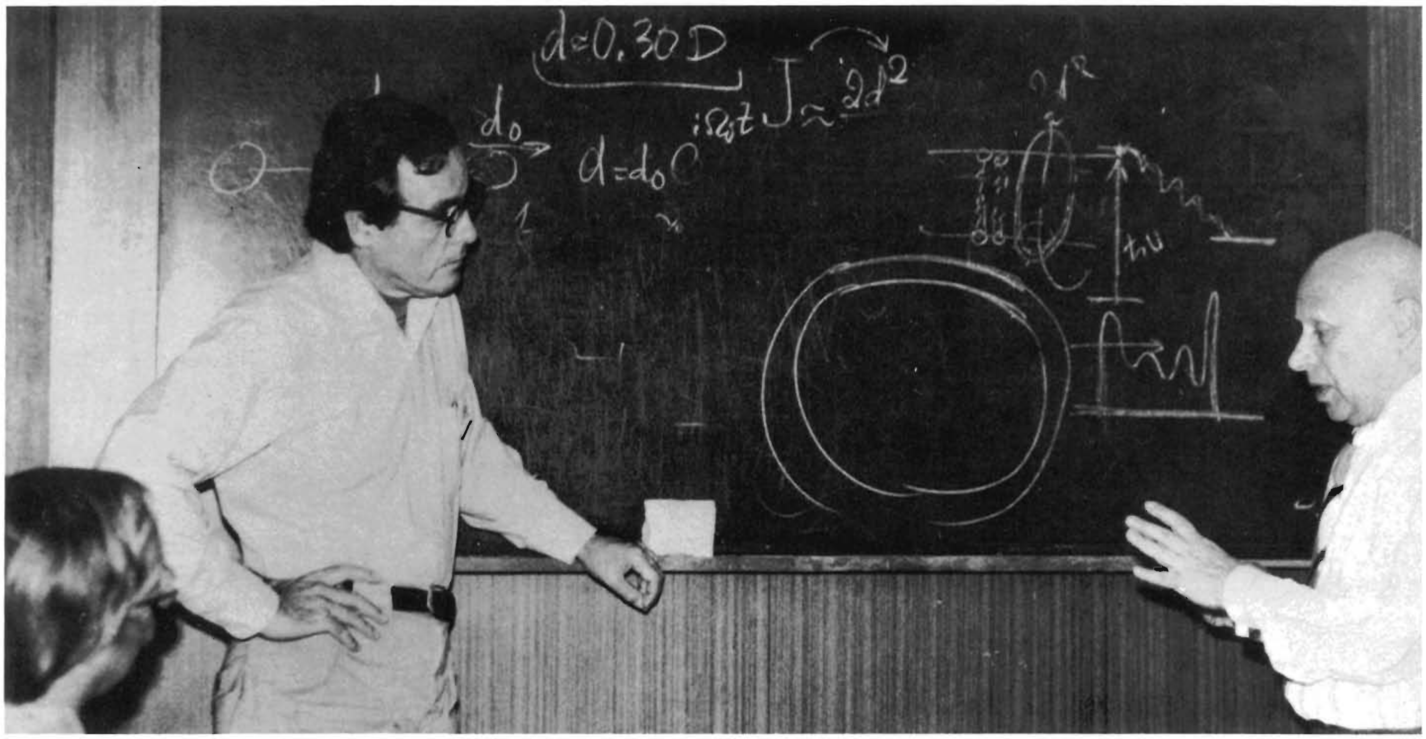
SCOTT: It's a whole change in thinking. A fundamental unanswered question in biology (labeled in 1973 the "crisis in bioenergetics") is how energy is stored, transported and transduced, and used to generate motion or change in form. Davydov showed how nonlinear concepts that are accepted in solid-state physics can be used in biochemistry. What he said seemed reasonable.

At the end of the conference Davydov gave me a stack of reprints and asked me to discuss his ideas with some biochemists here in the States. I tried, but I simply couldn't make clear to them what Davydov was talking about. So it seemed it might be useful to do some numerical calculations and show pictures, or a movie film or



Davydov showed how nonlinear concepts that are accepted in solid-state physics can be used in biochemistry.

INTERVIEW



something, of what was going on. Since I knew Mac Hyman and Dave McLaughlin at Los Alamos, we ran some calculations on the CDC 7600 computer here. These calculations started showing threshold effects that were interesting to investigate further, and that's how I was drawn into the problem.

SCIENCE: So originally you were to be a conduit between Davydov and some biochemistry people. Do you want to say some more about the ideas behind your current project and what you hope from it?

SCOTT: The impression I got from trying to communicate Davydov's ideas to the biochemists was that they are, in many cases, ill-prepared by their background and training to think about dynamical effects in large biochemical molecules like proteins or DNA. They know a lot about the structure of these molecules, but they are trying to go from a knowledge of structure to knowledge of function without thinking about dynamics. One thing I hope to see coming out of our project is a contribution from Los Alamos, with all its high-speed computers, to the understanding of nonequilibrium biochemical dynamics.

Of course, it's a dangerous quest because we physical scientists are very naive about biological reality. Physical scientists often come into some area of biological science and are just—you know—typically arrogant. But we do have good contacts with the Life Sciences Division. Mark Bitensky, their Division Leader, has been connected with this project since the beginning, and Scott Layne, one of our postdocs working on it, is an M.D.

SCIENCE: When I first heard about your project, I was intrigued that physical scientists were going into this biological area. Initially I wondered whether they just had some cute mathematical model they were somewhat arbitrarily trying to plant in these proteins.

SCOTT: This has been typical of what is called biophysics or mathematical biology. You find a physical scientist who has some idea or differential equation and then goes around in the biological world looking for a place to apply it, making all sorts of assumptions and approximations that aren't justifiable from the biologist's point of view. Biological scientists are sensitive to that; they're concerned about somebody who just waltzes into their field from the physical sciences. And rightly so, I think.

SCIENCE: In moving into this area, did you have any specific intuition in terms of the biological context? Or have you gained some?

SCOTT: I've always been interested in systems in the real world that are nonlinear, because they can do more interesting things. Of course, that makes them harder to solve because the unexpected can happen. When you look around in the biological world, essentially everything is, somehow or other, nonlinear.

Proteins, for instance, are marvelous molecular machines. This alpha helix that we are studying is a major constituent of hair. It's a long, helically shaped, fibrous protein, and you can readily see why its structure is appropriate to hair. In this case, going from structure to function is not a difficult leap to make. But if you think more generally of what is happening with proteins, of the many kinds of dynamic roles that they're playing all the time in every cell, then the leap from structure to function cannot proceed without considering the dynamics—the how. And immediately the thought arises that nonlinear effects are playing a vital role. It seems that it must be so—of course, that's different from proving it.

SCIENCE: What do you expect from this particular tack that's being taken, that is, solitons in proteins somehow transferring energy?

SCOTT: Well, in the first place, I would describe our work as more general than trying to study solitons in proteins. As I see it, Los Alamos Laboratory is in a unique position to analyze biological materials from the experimental, theoretical, and computational points of view. With the experimental physicists here and all their equipment on one side and the computing facilities on the other side, we can take material for which the structure has been determined, look at it in the laboratory, and then analyze it on the computers. I see that as broader than "looking at solitons in the alpha helix," although that was Davydov's original, seminal idea.

SCIENCE: So you'd like to look at solitons in materials in general?

SCOTT: Rather at nonlinear effects that are related to solitons. For example, the notion of a soliton makes sense if you think about a protein like this alpha helix as a major constituent of muscle. The protein runs along in a strand, and you can think about a region of excitation that travels along it, rather the way a pulse travels along a nerve fiber. However, if you have a globular protein in which the protein folds up on itself, the notion of a neat solitary wave may no longer apply. The dynamics may be related to what Davydov was talking about along a linear protein, but geometrically it will be a lot more complicated.

SCIENCE: Peter Lomdahl has some interesting numerical results for the globular protein lysozyme, but he is reluctant to discuss it in this issue.

SCOTT: There are four or five refinements that he'd like to have in the model, and those could easily change details of his results. One thing Peter's calculations do show is that nonlinear calculations for such a complicated object are not at all out of the question: it's something that can be done here at Los Alamos with our computing facilities.

SCIENCE: Are there practical engineering applications for biological solitons?

SCOTT: People are talking about "biochips" as a way to use biological molecules for computing at much higher density and smaller scale. And Davydov's soliton is one of the mechanisms that people are thinking about at the Naval Research Laboratory and in industry for doing the computing.

SCIENCE: How do you feel about that?

SCOTT: Optimistic—and, you know, throughout the history of scientific prediction, optimists have almost always erred on the conservative side. I see our work at Los Alamos helping provide the scientific foundation that will make these developments possible.

SCIENCE: I want to switch the topic now. You've traveled widely and worked at many places around the world. Why did you choose to come to Los Alamos?

SCOTT: Well, the Center for Nonlinear Studies started up with aims that I share. I've always been very keen on attempts to understand how nonlinear dynamic effects can play a role in real scientific problems, but for a long time this has been an area of science that was pushed into the background.

SCIENCE: Why was it pushed into the background? Because people couldn't make any progress?

SCOTT: It really wasn't fashionable until 1970 or so. There are a lot of fashions in science, as in other things. But now here is the Center with the aim of encouraging young people to get started in the field. I feel kind of psychically in tune with the charter of the Center.

SCIENCE: Can you give us more details about the aims of the Center?

SCOTT: As I see them? Sure. One is to increase, to improve contacts among nonlinear scientists at the Laboratory and in other parts of the country, particularly those at the universities, and in other parts of the world. The Center is running quite a number of workshops, a large conference every year, and sponsoring the visits of many fine scientists every year. A second aim is to stimulate nonlinear science inside the Laboratory, to start programs here that haven't been going on before, and to help branches of the Laboratory that haven't been interacting with one another in the past but could do so fruitfully. I think this biological project is a good example because it's bringing together people in the Chemistry Division and in the Life Sciences Division and people who are engaged in computational physics—they're all working together on the same project. A third thing is the one I mentioned: the Center is trying to get young people who are starting out in science into the area of nonlinear science, nonlinear dynamics. One of the important things going on in the Center is a vigorous postdoctoral program.

SCIENCE: What are the areas in the Laboratory where nonlinear dynamics is important and needs to be investigated?

SCOTT: Almost everywhere. The traditional areas of Laboratory activity—combustion and explosions—are fundamentally nonlinear. Energy released by a moving front in turn reacts to drive the front. The candle flame is a useful paradigm. Its heat releases energy (vaporized wax) from the candle, and this energy acts to keep the flame hot. And this process of nonlinear diffusion is just what is going on as a nerve impulse propagates along a fiber. Electrostatic energy

... now here is the Center with the aim of encouraging young people to get started in the field. I feel kind of psychically in tune with the charter of the Center.

stored in the cell membrane's capacitance is released and drives the pulse. Michael Faraday said once that the best way to begin understanding natural philosophy is to study the candle.

SCIENCE: What can the Center for Nonlinear Studies do to help in the area of understanding explosions? The Laboratory has had a large research program for forty years.

SCOTT: It does sound presumptuous when you put it that way. One question that I feel we can help with there—and only help with—is to determine the level of complexity at which, at the present time, it is possible to do real science. For example, for what mixture of suitably simplified explosive gases is it possible to write down all the equations and then to solve them using a combination of theoretical and numerical techniques, so that one can predict from first principles what's going on. And to determine where it is necessary to use rules of thumb and heuristic computational procedures, which may work well and be refined over a period of time but which do not proceed from first principles. I think it's important to understand where that line is, and people at the Center have been trying to help define it.

SCIENCE: That's interesting. Is there any group working in fusion, in plasma physics?

SCOTT: Yes, because this is another area where the physics is extremely nonlinear. People who have been attempting to design large energy-producing machines have been concerned for a long time with solving their nonlinear systems, with understanding what stationary states they might have and what instabilities might arise. Some of the work that has been going on in the Center is of interest to those people. The traditional approach has been to determine conditions for instability of particular stationary states. Recently Darryl Holm and collaborators in the University of California system have developed general techniques for determining stability of classes of stationary states. This is exciting and potentially very important.

SCIENCE: Do you have much interaction with the University of California?

SCOTT: Thanks for asking. Yes, we have a special program to encourage scientific collaboration between UC and the Laboratory. They have made funds available for faculty to spend time here, and we are helping Los Alamos staff members make arrangements for research visits to UC campuses.

SCIENCE: It's been suggested that people outside the Laboratory know the Center much better than people on the inside. How do people become associated with the Center?

SCOTT: There are various forms of involvement, from having a sympathy with the aims of the Center and some collaboration with one of the programs to being fully supported, somehow or other. There are postdocs; there are guests for various lengths of time; there are people inside the Laboratory being partially supported by money from the Center in order to work on certain projects. There isn't one typical form of association, and I think that's good. One of the nice things going for the Center is that the budget is flexible: thus it's possible to do whatever makes sense to do without concern for precedent.

SCIENCE: So projects just evolve, and people merge into them?

SCOTT: It's diffuse. A typical way for a person to get involved is that he or she has a subject for a workshop. We talk about it, and it is interesting to invite so and so from New York, so and so from Britain, so and so from California, and so and so from Peoria because these are the best people in the world in that field. We can decide to do that, and a month or two later they are here, talking to one another, and in some cases getting acquainted for the first time. People in the Laboratory who feel that they could become interested in the subject get drawn into the workshop and have the advantage of interacting with all these people. And maybe something starts up.

SCIENCE: And then there would be money to bring postdocs in that particular field to continue the work.

SCOTT: Yes, if it makes sense.

SCIENCE: Will you tell us some of the topics that are subjects of research at the Center now?

SCOTT: Well, we don't want to be totally unorganized, so we have several research themes which give directions to the effort. At present there are five themes: reactive flow; instabilities, material interpenetration and mixing; coherence and chaos in dynamical systems; polymers in synthetic metals; and energy transport mechanisms in biological polymers.

SCIENCE: What's happening that is really exciting?

SCOTT: We've talked some about the first two. The work on chaos is also very interesting at the present time.

SCIENCE: Why?

SCOTT: In many areas of science one wants to understand the dynamics of a dissipative system that has many degrees of freedom and that is being supplied with energy. One of the plasma machines, for example, or the surface of the ocean: on very large scales of distance and time the dynamics of the earth's crust driven by internally generated heat and on very small scales the dynamics of a protein molecule driven by metabolic energy. If the rate of energy input is raised above a certain level, the dynamics becomes chaotic. But its behavior can be described rather simply because the motion in the phase space remains on an attractor with a dimension that is often much less than the number of degrees of freedom of the system.

SCIENCE: Why is that important?

SCOTT: It means that the number of dependent variables necessary to describe the chaotic motion is much smaller than one would expect. In fact, numerical techniques are being developed at the Center to determine the dimension of the attractor from the chaotic motion of a single variable. Doyne Farmer and Erica Jen, working in collaboration with experimentalists at the University of Texas, recently found that the attractor for Couette-Taylor flow has a fractal dimension less

than five when the Reynolds number is 30% above the onset of chaos.

SCIENCE: You mentioned polymers as a research theme. Isn't this more appropriate to the Center for Materials Science?

SCOTT: It certainly is appropriate for them and, in fact, this effort is being carried on as a collaboration between the two centers. Roughly speaking, the theoretical work is in the Center for Nonlinear Studies and the experimental activities are in the Center for Materials Science. There has recently been some exciting experimental progress—Mahmoud Aldissi and Rai Liepins have reported the synthesis of polyacetylene in solution. This could be a key step in the commercial production of plastic metals. For example, it could be used to reduce the reflection of airplanes to radar, could be used in large but lightweight batteries for electric automobiles, etc.

SCIENCE: You have said that you like the work of the Center because it is really fun and worthwhile. You've also said that work in the nonlinear field is different than work in many scientific, technical areas today. Will you expand on that?

SCOTT: Well, in many ways World War II was good for science, for physics and mathematics in particular. After the war governments recognized that science was an important source of national power. For that reason they began to support science heavily, especially in the United States. But I don't see it as an unmixed good. What happened was that science changed from something that people did because they liked it—before World War II there wasn't any other incentive—into a reasonable way to make a living. And so although the quantity, the amount of scientific activity, has vastly expanded since World War II, the general level, and that certainly includes the morale, is not the same.

But nonlinear science was unfashionable, as I mentioned before, not the kind of work that *serious* scientists did. And so the people who were getting into this field in the late fifties and in the sixties were doing so not because they thought it was going to be useful for their careers but because they felt, regardless of what other people said, that the world really wasn't flat, that it was kind of curved. To these people it was necessary to do something that was important rather than to go along with everyone else.

So when people in nonlinear science finally (around 1972) began to get together and find out that they had been thinking the same ideas and working on the same problems for ten or even fifteen years, there was a real recognition there, a kind of family feeling. That feeling continues to set the tone of the activities in this area.

SCIENCE: How did you get into nonlinear waves?

SCOTT: I was an undergraduate in physics, and I remember at that time wanting to find some particle-like solutions for the electromagnetic equations. Maxwell's equations, of course, are linear, but it seemed that the electron should somehow be part of the field. Of course my efforts were totally unsuccessful. There wasn't any way of getting a localized dynamic entity as a solution of Maxwell's equations because the basic solutions of these linear equations extend over all space.

Then when I had completed my bachelor's degree in physics (and I made a solemn oath that I was never again going to be inside a university as long as I lived), I worked in New York City for a couple of years in an engineering job. I remember at that time having interest in the nerve problem: what was going on in the brain, what was really happening with the pulses? I can remember going into the library there on Fifth Avenue, past the lions, and getting Rashevsky's book out and reading it and starting to think about how nerves work. But you know, I had never learned or thought anything in a formal sense about nonlinearity.

A few years later I went back to graduate school and was working

What was missing from everyone's intuition . . . was the notion that because of the effects of nonlinearity, energy could organize itself, focus itself, into spatially localized packages. . . .

on an experimental thesis that involved making some very large Esaki diodes, tunnel diodes. (The Esaki diode is a semiconductor device that essentially shows a negative resistance on a surface.) I was making these things and analyzing them from a linear point of view. Then a Japanese visitor, Professor Nishizawa from Tohoku University, remarked that they were also analyzing large Esaki diodes, but from the point of view of nonlinear wave theory. I had never even heard those words before, but they rang a bell. That was in 1959, and I began spending a lot of time in the library, getting going on my own in the nonlinear direction. And also becoming somewhat angry over the sort of formal graduate training I had received, because although it was recognized that there were nonlinear effects in the world, the presumption was that these really weren't a problem. Whenever nonlinearities appeared, you just divided the nonlinearity up into piecewise linear sections, solved the linear problem for each section, and then matched these solutions at the angle points. That was all there was to it, nothing to worry about. That was the way it was taught, and that's completely wrong.

SCIENCE: Along that line, do you want to explain the nature of nonlinear phenomena? Why can't you just add up the pieces?

SCOTT: In a sense what one means by nonlinearity or linearity is a statement about cause and effect. If a dynamical process is linear and you try a certain cause, say cause A, and you get a certain effect, effect A, and then you try a different cause B and you get effect B, then doing cause A and cause B together gives you just the sum, effect A and effect B together. If a process is nonlinear, when you use cause A and cause B together, you get an effect that's not the sum of effect A and effect B.

A simple example of this is a match and a candle. You take a match (that's a cause A) and you light the candle (that's effect A). Then, after you put out the flame, you take another match (that's cause B) and you light the candle (and that's effect B, another flame). But if you put those two things together (if you light it twice), you don't get two flames—you get only one flame. The reason is that the effect is dynamically self-sufficient, and you can see that life itself is rather like that: once it gets started, it can continue on its own.

SCIENCE: Something about nonlinearity seems to go against people's common intuition, their accustomed thought patterns. I'm thinking of the Fermi-Pasta-Ulam computer experiment.

SCOTT: Well, in that case it wasn't going counter to just anybody's intuition—it was going counter to Enrico Fermi's intuition, which didn't very often happen. What was missing from everyone's intuition at that time was the notion that because of the effects of nonlinearity, energy could organize itself, focus itself, into spatially localized packages that would hang together even though they bounced around and hit the walls or hit each other. What was happening in that original, very germinal, computer experiment was that they started a numerical computation by putting all of the initial energy into the lowest mode of vibration on something like a violin string—the single, half-wave sinusoidal vibration. What they observed was that at initial moments of time this energy seemed to be spreading itself out into the various modes, but it eventually refocused and almost completely recaptured the original distribution of energy in the lowest mode.

Kruskal and Zabusky finally explained what was going on. They saw that this original placement of energy could be viewed as organizing itself into a number of soliton components. Since the soliton components would all have individual velocities and bounce back and forth in various ways, after a while the conditions would become just right for them to get reorganized into the positions they were in at the beginning. It would then look as if all the energy were back in the original mode, organized in the original way. But without the concept of nonlinear, self-trapped packages of energy, people just simply didn't have the tools to put that together. Of course, the concept had been around a long time, since 1834, but scientists never took it seriously.

SCIENCE: I'm thinking about the story of the man with the horse.

SCOTT: Yes, that one. I've done a lot of reading about that.

SCIENCE: Did that happen?

SCOTT: Oh yes, that happened, but the story about Russell and the horse gives a wrong impression, the impression that John Scott Russell was out riding one afternoon—maybe watching butterflies or picking wild flowers—and just happened to see this wave on the canal and followed it along for a while and then wrote something cute in his notebook. Actually, Russell, although only twenty-six years old at the time, was well launched into a career as a civil engineer. He was involved in many engineering designs, and among them was the question of redesigning horse-drawn barges for motor power because railroads were in competition with the canals. Russell organized a series of studies to measure the drawing force versus the velocity of a canal boat. Just a year or two earlier a peculiarity of the horse-drawn barge had been discovered: if the horse got the barge up to a certain speed, depending on the canal and so forth, and if you whipped him so that he jumped and got the boat going faster, then he'd actually have to pull less, do less work. Russell knew about this anomaly, and he had an elaborate experimental setup there on a canal near Edinburgh to pull the boat with a constant force, using weights and pulleys, so that he could measure the velocity very carefully. During the experiments one of the ropes broke, and the boat stopped. And that was when he saw this wave going off the front. He immediately jumped on a horse that was nearby and followed the wave, thinking of it as the source of the anomaly and something very important to understand. It turned out that the anomaly was caused by the boat interfering with the solitary wave it was creating. Russell went on to become preeminent in the nineteeth century as a naval architect.

SCIENCE: Where were we? I guess we were discussing the question of scientists taking nonlinearity seriously.

SCOTT: Well, my point is that Russell knew the solitary wave was important—all his life he knew that. Yet it was treated just as a peculiarity, of no scientific importance. If you look at Lamb's great book on hydrodynamics published in 1932, there are only three pages devoted to John Scott Russell's solitary wave. Yet Russell had insisted all his life that it was a revolutionary idea not just in hydrodynamics, but in acoustics and in electromagnetic waves and throughout physics.

SCIENCE: Is it a revolutionary idea? Is it having that impact now?

SCOTT: Well, of course I'm an enthusiast. But objectively I think anyone would agree that it has been revolutionary in solid-state physics since 1970. Just in understanding solids, these nonlinear states, once you start looking for them, are found in many, many different contexts.

Take, for example, a polymer like polyacetylene; in it there's a solitary wave that's essentially a charge transport mechanism. People hadn't recognized it was there before, and yet that's an important property of the material. It makes it possible to produce a lightweight material out of very cheap components, carbon and hydrogen, that has a high electrical conductivity and can store electrical charge. That's extremely important, but it wasn't appreciated, couldn't be appreciated until people had the notion that a nonlinear mechanism could self-trap charge or energy or magnetic flux, whatever is conserved in a particular context.

SCIENCE: Going back, why did you, when you received your undergraduate degree, swear that you would never go back into a university?

SCOTT: Hm . . . it felt like being in a box. I've always liked to work in a real scientific laboratory or to tinker, but I hated laboratory practice in school. You'd go into this situation where you'd just do the same experiments that everybody else had been doing forever. It wasn't a question of learning something or having fun; it was a matter of doing precisely this and getting the *right* answer.

SCIENCE: Did you always do both experimental and theoretical work?

SCOTT: When I first went to work with a bachelor's degree, I did

...throughout the history of scientific prediction, optimists have almost always erred on the conservative side.

primarily experimental work. I was working for a company that made microwave tubes, traveling wave tubes, backward wave oscillators, and things like that. I was making these things, making measurements on them, and doing a little theory, but not much. For a number of years I did mainly experimental work.

SCIENCE: And now you mainly do theoretical work?

SCOTT: Four years ago I spent a year doing experiments on electrophysiology at the Zoological Station in Naples. During that year I probably didn't write more than ten equations.

SCIENCE: So for you it hasn't been an evolution from one to the other, from experiments to theory?

SCOTT: I have always enjoyed the opportunity to do some of one and some of the other. It depends, of course, on what needs to be done. One of the reasons I was in Naples was that I had been interested for a long time in some theories of how the nerves work, but I hadn't been able to get electrophysiologists to do experiments that were directed that way.

SCIENCE: Will you tell us something about that work?

SCOTT: The experimental subject was a squid that is common in the ocean, particularly in the Mediterranean. It's a good subject because it has a very large nerve fiber. The aim of the experiment was to study the nerve pulses on this fiber, to treat them seriously as dynamic entities, and in particular, to see how they would interact with one another when they came together at branching points. One of the notions that people have had is that a nerve cell can actually do computing tasks at the branching points where the fibers come into the nerve cell, not just inside the body of the nerve cell. The aim of this research was to see whether the pulses could interfere with and cancel one another at branches.

SCIENCE: If these pulses were solitons, they would just go through?

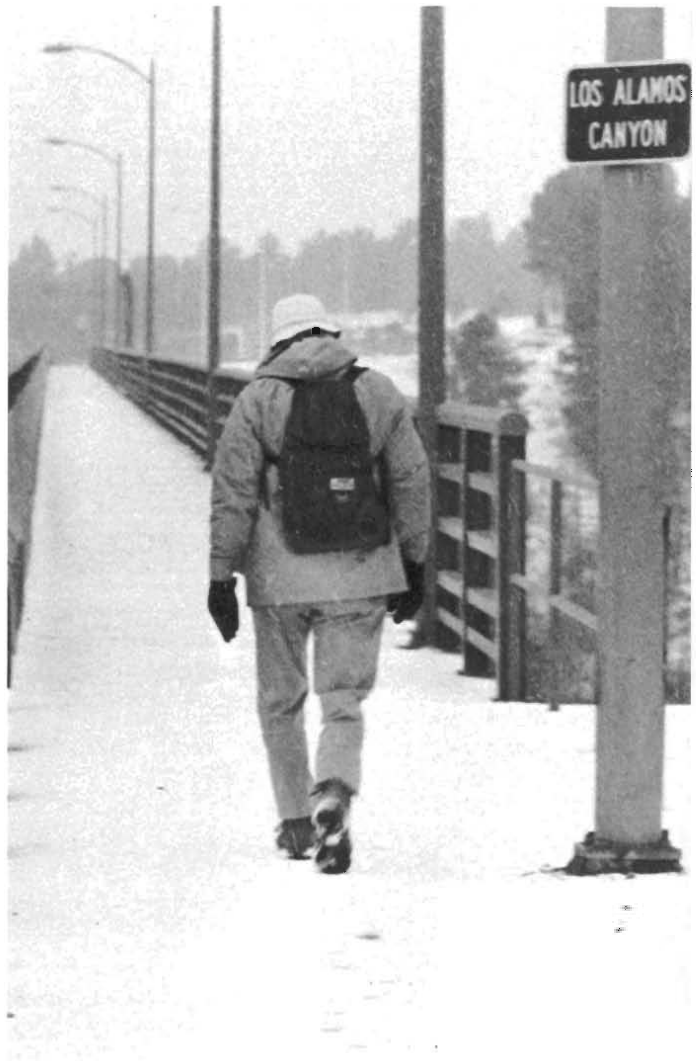
SCOTT: Yes, but they're not solitons because a nerve fiber is very much like the candle we were talking about, always releasing energy and dissipating it. Nerve pulses don't go through each other for the same reason grass fires or candle flames don't go through each other: they've exhausted all the fuel by the time two of them meet.

SCIENCE: What was happening at the branch points?

SCOTT: You can think about a situation where going toward the cell body you have two fibers that are merging into a single branch, like two branches merging into the trunk of a tree. And you can imagine a situation where the geometrical constraint is such that a pulse coming along one of the branches can't ignite the trunk, but two pulses simultaneously coming along both of the branches and getting to the trunk together can ignite the trunk. In computer terms you would speak of that as an *and* connection, because you need pulse A and pulse B in order to get an output pulse.

SCIENCE: Did the intuition for such problems, for working on nerves, come from your work on large Esaki diodes?

SCOTT: Yes, indeed it did. It turns out that the nonlinear wave process that takes place on large Esaki diodes is very similar to what is happening on a nerve fiber. That's when I first became interested in nerve fibers.



SCIENCE: Intuition has to come from some place, doesn't it?

SCOTT: Where motivation comes from is hard to say. I have the feeling that my motivation for the kind of work I'm doing has always been there.

SCIENCE: Then you had an interest in science in your childhood.

SCOTT: A ham set when I was fourteen, an interest in physics and chemistry in high school, lots of tinkering in electronics.

SCIENCE: I wanted to ask you what questions you're now interested in answering, especially in biology. Perhaps we've covered that already.

SCOTT: Biologists really have a whole set of interesting and difficult questions. And some things they are getting answers to. Knowing the structure of DNA and the genetic code is an enormous advance in biological knowledge. But think about the questions they're still faced with: here is an egg and a sperm that come together and start the development of a new organism—how does it somehow manage to take the form that it does? It's not a question of what's going on but of *how* it's going on. It's clearly nonlinear, of course, but the basis for it is not understood at all. That's exciting.

SCIENCE: I've been hearing scientists in many disciplines talking about fractal dimensions, and I associate that term with nonlinear problems and the work of the Center for Nonlinear Studies. I get the feeling that all of science will eventually be permeated by some new understanding about how to look at problems.

SCOTT: That's right, that's right.

SCIENCE: What do you see as the paradigms for modern research?

SCOTT: Well, the whole outlook that an applied mathematician or a physicist has now is totally different from what it was fifteen years ago—and in ways that couldn't have been imagined. In the mid-sixties the general feeling was that a nonlinear partial differential equation was so complicated as a mathematical entity that getting any kind of result beyond a numerical solution on a computer was hopeless. That was in the basket of things that everyone had given up on. Essentially through the work of Kruskal, it became apparent that you could take nonlinear partial differential equations and find very simple ways to analyze them and get exact answers. In a certain sense Kruskal's solution procedure was analogous to solving linear equations. He showed that solitons are a nonlinear generalization of the idea of a Fourier component, or a normal mode of a linear system. In other words, a general solution to a certain class of equations could be rigorously constructed by using solitons as the constituent entities. So, many problems considered hopeless turned out in many cases to be, if not trivial, at least quite do-able and lots of fun. The Fermi-Pasta-Ulam problem was what set Kruskal on the road to working out this concept.

On the other hand, all through the sixties it was considered that ordinary differential equations with two or three independent variables were so simple to solve that there wasn't any reason to be interested in them. And then during the seventies people recognized that a very simple system of three ordinary differential equations with a little nonlinearity added can become chaotic and can no longer be solved in a predicted manner on the most powerful digital computers.

At that point everything was turned topsy-turvy. The problems that were considered so simple to solve that they weren't even worth looking at turned out to be impossible to solve with the best numerical machines we have because they led to chaos. On the other hand, the problems that were considered impossible to solve turned out to be easy because their solutions could be constructed from solitons. The landscape is totally different now than it was fifteen years ago when you consider the kinds of problems we are interested in working on, what sort of tools we use, and what we expect to happen.

SCIENCE: I always thought of the soliton as a curious solution to a particular equation, not something that you would necessarily expect to find in nature. But you are suggesting that it may be quite common.

SCOTT: Yes, the soliton, or more generally the self-trapping of energy or charge into solitary waves, is really a paradigm. It has been around, you might say as a latent paradigm, since 1834, but it began to be recognized only around 1970 as something a typical scientist might expect to find in a typical problem. And the recognition is growing. Every month or so some scientist discovers yet another equation that has soliton solutions. On the other hand, people whose training is entirely with linear equations think of what is going on as a kind of Fourier reconstruction of sine waves without trapping.

SCIENCE: This soliton paradigm seems to evoke an emotional response, with some people *believing* in solitons and some not.

SCOTT: My feeling is that that's the way these changes work. Think of somebody who has spent his entire life developing a certain point of view and has a substantial position in the power structure of science—and then some young people come along with new ideas that he doesn't understand at all, ideas that, if true, would be harmful to his historical position and present power. He's in a position not to believe.

SCIENCE: What do you think of in terms of training people to work on nonlinear problems? What kind of training would be best for them?

SCOTT: Well, for a long time there has been a need in physics graduate programs for a good course in nonlinear science. Many graduate schools are getting something like that now. Probably it's not necessary to have too much more than an introduction to modern concepts. The most important thing is the thesis: many people get pointed in the direction of nonlinear science by the choice of their thesis problem.

SCIENCE: There seem to be some problems in communication. Some people, especially in the biological and biomedical disciplines, seem to be throwing around the word *soliton* without knowing precisely what it means. (We hope to clarify its meaning in this issue.) Then there are the language barriers that exist between disciplines.

SCOTT: Yes, there are language barriers and also concept barriers. But exchange between disciplines has been fruitful in the past: x-ray crystallography is one example, another is nuclear magnetic resonance, and still another is neutron scattering. Solitons may be another area where the interaction between biochemists and physicists could be important. If Davydov's ideas are essentially correct, then they will be important for understanding the way proteins function; if not, then the work will fizzle out in a year or two. But I won't believe that

So, many problems considered hopeless turned out in many cases to be, if not trivial, at least quite do-able and lots of fun.

nonlinear dynamics will not have a role to play in understanding how biochemicals function—that much seems certain.

SCIENCE: It seems the start of an exciting venture. Does the work on solitons dovetail with the programmatic work already going on in Life Sciences?

SCOTT: Yes, indeed. For example, Mark Bitensky has studied for a number of years the succession of dynamic events that take place after a photon of light comes into the eye—all of the activity in the protein rhodopsin before a pulse is perceived as a signal on the optic nerve. At the present time, as I understand it, much is known about *what* is happening with the various components of rhodopsin. Just *how* they're happening and *why* is not very well understood. I think Mark is interested in our work because it may provide a basis for answering some of those questions. In fact, the life sciences are just dripping with such questions.

SCIENCE: How do you find the intellectual climate of Los Alamos? How do you feel about having this as a place to work?

SCOTT: Well, all large organizations have some problems just because they are large and complicated. But it seems to me that there are interesting differences between the social dynamics of a university and those of a national laboratory. It's been particularly impressed upon my mind since I came here from the University of Wisconsin, which is one of our larger educational institutions.

In the university the essential group, the cellular unit, is the department. The administration of the university can't penetrate the department very much. To eliminate a department would take an act of the state legislature, so a department has the possibility of challenging or even ignoring directives of the administration. Thus there is a difficulty getting departments to interact with one another and in organizing projects between departments. Which dean will control the project? Where is the money actually going to go? What is the advantage of one department giving aid to another department?

Here at the Laboratory we have a strong administrative component on the one hand and a large scientific staff on the other, but the groups here don't have the same legal status and coherence that characterize departments in a university. Here it is relatively easy to get people in widely different disciplines together on the same project; it can be organized in a matter of weeks. It's a great advantage to be able to get biochemists and laser physicists and computer scientists and mathematicians all working on the same project.

SCIENCE: How about the financing of the Center? Since you are funded by the Laboratory, how much autonomy do you have? Is the money yours to use, without strings attached?

SCOTT: To use *responsibly*. And it's interesting that to deal with the charge of the Center for Nonlinear Studies to use this money flexibly and imaginatively precludes doing those things that lead to bureaucratic stability. If we were opting for that, we would try to convert the budget into permanent positions, not use it for postdocs, visitors, workshops, and staff support throughout the Laboratory.

SCIENCE: As the Center's many projects become established, it seems that the postdocs who come to work on them might want to stay here.

SCOTT: Well, more than half the postdocs who come to Los Alamos do stay. If the Center draws good postdocs to the Laboratory and they go into other groups and divisions, that will certainly benefit the Laboratory.

SCIENCE: And they will presumably continue their interest in nonlinear phenomena.

SCOTT: Yes, and those who go into the universities will also seed interactions between Los Alamos and the universities.

SCIENCE: Have the divisions offered to donate their own money toward these projects? Say someone in a division wants to work on a project that relates to the Center, will the division support that person rather than your having to?

SCOTT: Sure. For example, in connection with the biology project, there's a significant component going on under the direction of Irving Bigio in the Chemistry Division. It gets some support directly from Chemistry and some support from an Institutional Supporting Research and Development request that is apart from the budget of the Center. And we hope, of course, that in the future some support will come from outside.

SCIENCE: How is communication among the people at the Center? Do you serve as an intellectual guide to the work that is going on?

SCOTT: That's hard to say because there are so many things going on, so many different kinds of activities. I'm in a position to act as a guide in areas that I know something about. But, for example, I don't know very much about chaos business. I try to keep up with what's going on, but there isn't any way I could be described as an intellectual leader in that area.

SCIENCE: But you do fulfill that important function of deciding where the money will go, what the aims of the Center are, and how those aims will be implemented—for example, the emphasis on, belief in, young people.

SCOTT: Well, it's important to have taste. I feel good about this kind of scientific development, and to be in a position to help manage it in a responsible way is something very satisfying to be involved with.

SCIENCE: We've talked about *belief,* that some people believe in solitons and some don't. There must be a certain sense of mission at the Nonlinear Center.

SCOTT: I would prefer the phrase *understand solitons*, but you are right about the sense of mission, and that is extremely important. The Center for Nonlinear Studies is prospering because many people here at the Laboratory and throughout the country recognize it as a good idea and have been willing to work to help it get started. That's absolutely vital: without this commitment from many people the Center wouldn't fly at all. ■

☆ U.S. GOVERNMENT PRINTING OFFICE: 1984 — 776-101/1025

LA-UR-84-430